Identifying and Tracking Sediment–Adsorbed Mercury Through the Lower Sacramento Valley

Michael Singer

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0090: Identifying and Tracking Sediment–Adsorbed Mercury Through the Lower Sacramento Valley

Funding:

Do not fund

Initial Selection Panel (Primary) Review

Topic Areas

- Environmental Influences On Key Species And Ecosystems
- Relative Stresses On Key Fish Species
- Processes Controlling Delta Water Quality
- Water Management Models For Prediction, Optimization, And Strategic Assessments
- Assessment And Monitoring

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

This project could be seen as relevant to all of the topic areas checked in the list, but obviously the relevance to some is quite indirect. How important Hg levels from this source are in determining water quality, public health hazards, and eocsystem effects in Delta water is not going to be determined by this project alone, but all of of general concern to CALFED.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

None of the concerns expressed by the three technical reveiwers that relate to budget are fatal (e.g. a better correlation between budget and work plan components, and why the $43K for a GPR?), and could be negotiated. More important are the concerns noted in their three reviews and as summarized in the TSP review (see below).

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

This proposal has merit and addresses a subject of interest and importance to CALFED but the overall weight of concerns by the reviewers is just too heavy to ignore. We have three pretty thorough reviews (thorough by comparison with other proposals reviewed in this process), and combined they raise questions about the (a) leadership of the team; (b) whether there will actually be enough attention to mercury in the project or expertise on the team to do so; (c) a poor match between budget and what is proposed; (d) the absence of predictive model as a product of the project, etc. Without discussing all of them again, it is my view that this proposal is just not good enough to fund at a $1Million level given the precious little funding available this year for the entire program.

Selection Panel (Discussion) Review

fund this amount: $**0**
note:
do not fund

The project proponents hope to track mercury movement through the Sacramento River, from its source in mine-spoils in the Sierra foothills to the SF Estuary. The Panel agreed with the comments of earlier reviews that this project will really track sediment movement with inadequate attention to mercury mobilization per se, although the Panel also saw merit in the proponents' desire to determine where mining sediment

accumulates within this watershed. The Panel shared the concerns of previous reviewers who identified several problems of the research plan and with bridging the gap between this research and its applicability to ecosystem managers.

Technical Synthesis Panel Review

Proposal Title

#0090: Identifying and Tracking Sediment–Adsorbed Mercury Through the Lower Sacramento Valley

Final Panel Rating
above average

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The proposed work focuses on sources, pathways, and sinks of hydraulic mining sediment likely to bear mercury, with a focus on deposition and resuspension in bypass channels. Work is appropriately presented in the context of a sediment mass balance, although the ability of the authors to close a sediment budget with sparse data over large time and space scales can be questioned. The work includes efforts focused on defining in the field sediment sources in the piedmont and sediment sinks in river floodplains and Sacramento R bypasses. Work on mercury is placed within the context of the sediment sampling and mapping. The proposal received three reviews, all informed, constructive, and useful. All felt the topic was clearly important, that the potential results, although initially oversold, were very likely to add useful information which will be, at least, a first step toward understanding the mercury contamination problem. All three reviewers thought the proposal was heavily focused on sediment and that additional geochemical expertise, particularly in the speciation of mercury, would have been useful. Concerns with the approach include (i) doubts about whether hydrologic records and deposition observations can be linked without a predictive routing model, which was not included in the proposed work (ii) inadequate discussion or acknowledgement of uncertainties

and how they will be used to determine reliability of results (iii) an absence of event sampling (iv) no proof of concept demonstration for the multiple fingerprinting approach (v) insufficient explanation of how the publications and GIS map would actually be used or useful to managers All three technical reviews assigned a value of "good" – right in the middle on their scale of five. The panel uses a scale of four and the rating pushes toward the higher side of average: "above average". This score is based on the definition used for the category: at least high technical and scientific value and will add a solid basic knowledge/understanding of the topic. The need to better understand sources, pathways, and fate of Hg contaminated sediment is clearly important within the CalFed efforts and the investigators have very strong credentials, good experience, and an admirable track record of innovative and productive research. Although the reviewers expressed various concerns about the approach, all accepted that its basic approach was sound and likely to produce important results. The reviewers mention a "strong and interesting" and a "generally strong" proposal and a project team that is "very capable" with "extensive experience".

Additional Comments:

The proposed work focuses on sources, pathways, and sinks of hydraulic mining sediment likely to bear mercury, with a focus on deposition and resuspension in bypass channels. Work is appropriately presented in the context of a sediment mass balance, although the ability of the authors to close a sediment budget with sparse data over large time and space scales can be questioned. The work includes efforts focused on defining in the field sediment sources in the piedmont and sediment sinks in river floodplains and Sacramento R bypasses. Work on mercury is placed within the context of the sediment sampling and mapping. The proposal received three reviews, all informed, constructive, and useful. All felt the topic was clearly important, that the potential results, although initially oversold, were very likely to add useful information which will be, at least, a first step toward understanding the mercury contamination problem. All three reviewers thought the

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The technical reviewers and the panel believe that the team has the capabilities to perform the study, and that this study would result in useful information and did not have significant technical shortcomings. However, in the absence of a routing model, products would not include a strong predictive model. The study is ambitious in its scope, and is likely to yield valuable information on fine sediment sources and fate, but less likely to yield a definitive Hg risk map. A

good first step but will need to be followed by a study with more complete treatment of Hg transformation and fate. In addition, there could have been more detailed discussion of uncertainties and of the sampling methodology.

Rating: Above Average

Technical Review #1

proposal title: Identifying and Tracking Sediment–Adsorbed Mercury Through the Lower Sacramento Valley

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	ARE THE GOALS, OBJECTIVES AND HYPOTHESES CLEARLY STATED AND INTERNALLY CONSISTENT? The goals and objectives are clearly stated in section A.1 of the proposal, whereas the hypotheses are found in section A.3. These objectives and hypotheses all build off ideas found in the literature, which are well-documented by the applicants, and previous work of various members of the applicant team. They clearly and consistently stick to these throughout the proposal. However, because there are so many specific objectives to tackle the true complexities of mercury contaminated sediments in the Sacramento system, I found it easy to lose site of the fundamental goal of this project: a source-to-sink sediment budget for mining sediments. IS THE IDEA TIMELY AND IMPORTANT? The applicants have selected a very topical and important area of research in the Sacramento River Basin restoration context. Mercury contaminated sediments present one of the big 'unknowns' in the Sacramento system. I am sceptical that a 'risk map' is the ultimate best use of the information potentially produced by this proposal. To produce such a map implies that we adequately understand the risks associated with mercury contaminated sediments under different scenarios: in situ storage, fluvial remobilization, or anthropogenic remobilization (e.g. restoration, removal, reworking

	or flood control). This is not something that this proposal covers (not that it should… its scope is already quite ambitious). What this proposal does promise is an improved understanding of the movement, residence times and distribution of mine-contaminated sediments. This is an obvious crucial piece of the larger restoration and management puzzle, and arguably an important first step to take. I would simply discourage any expectation that the proposed research will produce the answers to the mercury contamination problem. The applicants rightly argue that this information could inform decision making, but only within an adaptive management context.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	IS THE STUDY JUSTIFIED RELATIVE TO EXISTING KNOWLEDGE? The applicants highlight a large knowledge gap with respect to the dynamics of mining-contaminated sediments. Fortunately for the applicants, this problem is presumably simplified by the assumption that contaminated sediments behave just like 'normal' sediments that geomorphologists typically study. Thus, applying a host of standard protocols to essentially investigate sediment dynamics (which we happen to know a fair bit about) and tacking some analysis of actual Hg concentrations onto it forms the backbone of this study. While there is nothing to suggest that this is a flawed approach, it is important to recognize that this is fundamentally a geomorphic study on sediment dynamics that happens to have implications (through inference and empirical evidence) on contaminated sediments. Being a geomorphologist myself, I would probably do the same thing. Moreover, this sort of analysis has not been done in this detail for this

portion (or any part… to my knowledge) of the Sacramento River System. Thus, the research is well justified. However, from a restoration and basin management perspective, I would emphasize that this research project actually opens the door to a host of other ecological issues we know even less about (the sediment dynamics, while crucial, are likely to be easier to deal with).

IS A CONCEPTUAL MODEL CLEARLY STATED IN THE PROPOSAL AND DOES IT EXPLAIN THE UNDERLYING BASIS FOR THE PROPOSED WORK? The applicants rely heavily on the work of Gilbert and James (part of project team) and use their conceptual models as the basis for the proposal. The applicants creatively craft their proposal around testing, quantifying and expanding on the qualitative information within these conceptual models.

IS THE SELECTION OF RESEARCH, PILOT OR DEMONSTRATION PROJECT, OR A FULL-SCALE IMPLEMENTATION PROJECT JUSTIFIED? This point is not really addressed within the proposal. It seems the author's view this as a full-scale implementation project without explicitly stating it. One question this proposal raises is the extent to which the information and techniques employed in this project will be applicable to other hydraulically mine systems within the Sacramento River System? They chose to focus on the Bear and Yuba, where the largest amounts of hydraulic mining took place. What about the other tributaries?

Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	

IS THE APPROACH WELL DESIGNED AND APPROPRIATE FOR MEETING THE OBJECTIVES OF THE PROJECT? The approach is generally well designed and provides specific and realistic techniques for meeting the project objectives. I like how the authors have tried to systematically approach this problem of contaminate sediment dynamics from a number of different angles. The presumption and hope is that all these different perspectives will converge on a coherent story and slight improvement over a first order sediment budget. It is certainly plausible, given the variety of techniques and the data sources the applicants are using, that totally divergent or inconsistent results could emerge. While the track record of these applicants suggests that they would be creative enough to make some sense of potentially incoherent results, I was disappointed by the lack of detailed consideration for such problems.

For example, there seems to be little consideration for the numerous unreliability uncertainties surrounding making comparisons of a variety of existing data sources (collected with different methods, for different purposes at different spatial and temporal resolutions). The applicants will be forced to choose some techniques (introducing structural uncertainties) to interpolate or extrapolate between spatially inconsistent data to make their analyses (e.g. DEM differencing between an old plane table survey, an aerial topographic survey and a LIDAR survey). Fundamentally, this is a process of temporal and spatial averaging. Especially when applying these techniques over long reaches (upwards of 40 km), small errors could potentially propagate to order of magnitude differences in volumetric estimates of sediment, for example. This is not to suggest that I disagree with the applicant's approach to their problem. I would simply like to see a more explicit and realistic outline of how they might transparently deal with these uncertainties. I would strongly discourage the applicants from interpreting this as

they should spend a lot of effort attempting to reduce these uncertainties. Uncertainty can be valuable information that, instead of ignoring, could be used to bound the plausibility of their results. In fairness to the applicants, they did mention in a few places that basic error analysis would be under taken (e.g. pg 19- 1st paragraph; pg 21- 1st paragraph). Moreover, extensive discussion of uncertainty in a page-limited proposal may not seem like a desirable topic to place emphasis on. I expect that in the reporting of their findings this could be rectified in a manner to help decision makers understand the ramifications and significance of the uncertainty in the findings of this study.

IS THE APPROACH FEASIBLE? The approach is certainly feasible in that it draws on a number of well-established techniques to accomplish the project objectives. Further, the applicant team has extensive experience with many of the techniques that their approach calls for.

ARE RESULTS LIKELY TO ADD TO THE BASE OF KNOWLEDGE? There is no question that this study will be of interest to the scientific community. The data from this study will purportedly be made available on the web. This should be a helpful resource for other investigators to mine for future studies.

IS THE PROJECT LIKELY TO GENERATE NOVEL INFORMATION, METHODOLOGY, OR APPROACHES? The information will certainly be novel. I don't see much in the proposal (other than using brut-force to tackle a very high-magnitude scope) that suggests any necessarily new or novel methodologies will come out of this study. This is not to suggest that the applicants might not subsequently develop some new methodologies. What really makes the proposal novel is the scale it will be applied at and the area in which it is being applied. Also, see above.

WILL THE INFORMATION ULTIMATELY BE USEFUL TO DECISION MAKERS? The information and findings from this study could ultimately be very useful to decision makers. However, the burden of making that information useful (as opposed to a liability) will lie heavily on the applicants. I've already commented on my reservations about providing risk maps to decision makers as an output. I think such risk maps would need to be explained quite carefully to decision makers as opposed to giving them a colourful risk probability map that says "stay away from the dark red areas." What explicitly is meant by risk (i.e. in the applicant's case – risk of erosion of contaminated sediments) is something that could easily and understandably be misinterpreted by decision makers. The applicants may wish to consider the unintended consequences of putting the words erosion and risk in the same sentence. To be more explicit, the implication that erosion of Hg contaminated sediment is a risk does not explain how or why it could be a risk. The 'erosion control' industry, which is primarily concerned with 'controlling' excessive erosion of fines from construction and agricultural sites, has already instilled a generally negative view of any erosion in the regulatory consciousness. Of course, any geomorphologist would argue that erosion is a natural and essential process to maintain a healthy ecosystem. So this raises a rather naive question, what is the risk posed by contaminated sediments not being eroded? Could there be unforeseen consequences of encouraging decision makers to view erosion as a bad thing? Or put another way, could there be unforeseen benefits to eroding contaminated sediments? Most scientists who have worked with the mercury contamination problem would probably quickly dismiss these questions as 'stupid'. Perhaps they are, but I would not consider a decision maker stupid for asking such a question. Science rarely provides answers… just more refined questions. Decision makers sometimes confuse 'decision support systems' as 'decision making systems'. I would be careful here.

	`Also, see comments above.`
Rating	`very good`

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	`IS THE APPROACH FULLY DOCUMENTED AND TECHNICALLY FEASIBLE? The applicants have done a fine job of providing relevant references to the literature throughout the proposal. I was unaware of many of the studies they cited and found some of them very interesting and helpful for elaborating on some of the techniques they were proposing. The approach is technically feasible. The only concerns I have about technical feasibility relate to their ability to actually combine the plethora of existing data sets they cite in a meaningful way. My experience with data-mining exercises is that you always get something, but it is rarely what you expected or hoped for. From looking through the approach, it doesn't seem that these hurdles would necessarily derail the project. The project is diversified enough in its approach to be able to adapt and recover from such problems reasonably well.`  `WHAT IS THE LIKELIHOOD OF SUCCESS? High. 'Success' is a subjective construct that if intended to be objectively measured is heavily dependent on specific criteria (that are not clear to me from CALFED). Thus, in my totally subjective opinion, the applicants are likely to be successful at completing the project in some form and disseminating the results widely.`  `IS THE SCALE OF THE PROJECT CONSISTENT WITH THE OBJECTIVES AND WITHIN THE GRASP OF AUTHORS? Yes.`
Rating	`very good`

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	ARE PRODUCTS OF VALUE LIKELY FROM THE PROJECT? Absolutely… especially from a geomorphologist's perspective. ARE CONTRIBUTIONS TO LARGER DATA MANAGEMENT SYSTEMS RELEVANT AND CONSIDERED? Yes. The contributions to larger data management systems from this project come in two forms. First, by mining a bunch of existing data sources and putting these into to more useful formats, they will contribute to a 'larger data management' system. Secondly, through making this data and their own data available via the web, the data could be used by anyone who wants to. ARE INTERPRETIVE (OR INTERPRETABLE) OUTCOMES LIKELY FROM THE PROJECT? See my comments in the approach section. My primary concern with this project is not that the interpretive outcomes of the project will not be interesting, but that they could be easily misinterpreted. This is not to suggest that the work shouldn't be done! Instead, it is important for the outcomes of this project not to be sold as something they are not (i.e. answers to the Hg contamination problem). Instead, they are crucial insight into the Hg contamination problem.
Rating	good

Additional Comments

Comments	Less for CALFED and more for authors: 1. For expressing uncertainties, you may wish to consider other techniques in addition or as alternatives to standard probabilistic approaches. Fuzzy set theory (e.g. fuzzy classification and fuzzy inference systems) provide some robust and flexible quantitative tools for dealing with the types of vague and ambiguous concepts and data sources you are dealing with. 2. DEM differencing techniques for sediment budgeting have evolved rapidly in recent years (the morphometric approach). You may well be very familiar with this literature but as there were no references to it in your proposal, I will assume you might not have seen some of these studies. Most practitioners and investigators still ignore uncertainty in DEM differencing estimates. Gaeuman et al. (2003) have developed some interesting techniques for accounting for uncertainties that would apply to the portion of the budgets that are derived from a combination of aerial photographs and field estimates of deposit depths from exposed cuts. For differencing DEMs derived directly from topographic surveys, many have adopted a minimum level of detection approach. Essentially, it is argued that below some threshold (10-15 cm for ground-based surveys; 20-30 cm for aerial surveys) elevation changes can not be distinguished from noise and therefore discarded. This is typically done by assuming that surface representation 'errors' are spatially uniform (e.g. Brasington et al., 2000; Lane et al., 2003). Lane and Chandler (2003) and Brasington et al. (2003) provide some interesting reviews of some of these issues. Some preliminary work was presented at AGU by Brasington et al. (2004) where they considered the spatial variability of the uncertainty and explicitly accounted for its influence on the budget. 3. As you consider your abilities to detect erosion versus deposition, you may want to think about the ability of your measurement or estimate techniques to detect

these two processes. If there are errors in the detection technique, do they apply equally to both deposition and erosion processes? Brasington et al. (2003) suggested that deposition tends to occur in broader, flatter sheets whereas erosion is often concentrated in a manner that produces larger elevation differences. The extension of this is that you can probably detect erosion pretty easily (e.g. Figure 3 from your proposal), but your ability to detect deposition may be much less (particularly in the floodplain environment). In a volumetric sediment budget, erosion and deposition volumes are customarily give equal weight (i.e. subtract one from the other). Is this necessarily appropriate?

The references I cited in the additional comments section are provided below:

Brasington J., Langham J., Rumsby B., 2003. Methodological sensitivity of morphometric estimates of coarse fluvial sediment transport. Geomorphology. 53(3-4), 299-316.

Brasington J., Rumsby B.T., Mcvey R.A., 2000. Monitoring and modelling morphological change in a braided gravel-bed river using high resolution GPS-based survey. Earth Surface Processes and Landforms. 25(9), 973-990.

Brasington J., Wheaton J.M., Williams R.D., 2004. Sub-Reach Scale Morphological Interpretations from DEM Differencing: Accounting for DEM Uncertainty. Eos Trans. AGU. 85(47), Fall Meeting Supplement, Abstract H43A-0352.

Gaeuman D.A., Schmidt J.C., Wilcock P.R., 2003. Evaluation of in-channel gravel storage with morphology-based gravel budgets developed from planimetric data. Journal of Geophysical Research-Earth Surface. 108, 1-16.

Lane S.N., Chandler J.H., 2003. Editorial: The generation of high quality topographic data for hydrology and geomorphology: New data sources, new applications and new problems. Earth Surface Processes and Landforms. 28(3), 229-230.

Lane S.N., Westaway R.M., Hicks D.M., 2003. Estimation of erosion and deposition volumes in a large, gravel-bed, braided river using synoptic remote sensing. Earth Surface Processes and Landforms. 28(3), 249-271.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	WHAT IS THE TRACK RECORD OF AUTHORS IN TERMS OF PAST PERFORMANCE? The track record of the applicant team is impressive and suggests that they are likely to successfully deliver this project (roughly as proposed) and disseminate the results widely. I have no doubts about their capabilities. IS THE PROJECT TEAM QUALIFIED TO EFFICIENTLY AND EFFECTIVELY IMPLEMENT THE PROPOSED PROJECT? Yes. The project team is intimately familiar with their proposed study sites and region - through previous research. Most of the project team have worked together on past projects as well and have demonstrated their ability to effectively implement projects. DO THEY HAVE AVAILABLE THE INFRASTRUCTURE AND OTHER ASPECTS OF SUPPORT NECESSARY TO ACCOMPLISH THE PROJECT? The proposal does not require too much that the applicants would not already have available between their respective institutions. Those items that are required for are reasonably accounted for within the budget.
Rating	

	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Yes. In some parts of the proposal I found it difficult to infer whether the budget would be adequate to cover what the applicant's proposed to do. This is primarily because the applicants emphasized the technical details of the methodologies, which they are to employ, without adequately describing the magnitude and spatial coverage of the sampling scheme. Working backwards from the budget items and knowledge of how long and how much many of the field and laboratory techniques proposed cost, I was able to make some inferences about what is practically feasible with the time and budget resources allotted. Based on this, I would say that the work actually proposed is much smaller in scope than what I was led to believe in the executive summary and introduction to the proposal. However, what is proposed would certainly be a worth-while contribution to the scientific and restoration communities and the proposed budget is a perfectly reasonable amount to accomplish this with. The applicants might be overly-optimistic about the time involved in acquiring and processing data from existing data sources. These are unlikely to have an influence on the budget as they will probably be absorbed in excess hours and headaches for some poor graduate or under-graduate student (or perhaps one of the PIs).
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	This is a generally strong proposal from a very capable project team that is likely to produce some interesting and topical findings. The proposal is a bit 'over-sold' on the front end with repeated claims as to the 'unparalleled' quality of the datasets to be produced. Such bold-claims early on certainly encouraged a more thorough review than would be otherwise necessary and detract from the otherwise high quality of the proposal. After reading beyond the 'sales-pitch' and into the depth of the proposal, what the applicants are proposing to do is still quite interesting and actually feasible. The research proposal does not in my opinion live up to its early claims, such as 'broad impacts for society as a whole'. Also, the application emphasizes the 'increased spatial scope and resolution' of this study over existing studies with vague claims - implying that some incredible 3D time series of maps (similar to Figure 11 in Poole et al, 2002) of the entire Feather and Yuba system will emerge from this research. In reality, the proposed sampling scheme does what is reasonable and expected- sacrifices spatial resolution for a broader spatial scope (albeit more detailed than past studies). Again, if one is to ignore these over-statements sprinkled throughout the proposal, a much more modest, yet strong and well thought out proposal emerges. It is the substance of this proposal that makes it worthy of funding, and the fluff that detracts from it. Best of luck.
Rating	good

Technical Review #2

proposal title: Identifying and Tracking Sediment–Adsorbed Mercury Through the Lower Sacramento Valley

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	– Overall, the idea presented is timely and important. The production of large quantities of sediment via mining, delivered directly to the rivers and streams of this region is well known and documented. The widespread use of Hg in gold mining, its subsequent release into the environment and toxicity is also well documented. A specific link between the upland sources of Hg laden sediments and deposition on lowland floodplains would be useful. If, as the investigators state, no such work has been attempted with results made available to the public, then this work is important. – Goals and objectives identified by the investigators are very ambitious given the temporal and fiscal constraints of the program.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	– Yes, the study is justified relative to existing knowledge. – The conceptual model is clearly stated, the combination of field and laboratory techniques is adequate to the series of tasks described. The work is

	sprawling in layout, but the fact that the investigators all have documented work experience in the study region lends credibility to the proposal. - The investigators cite a number of papers in press or in submission and alude to the collection of a significant amount of preliminary data (cores, sediment samples, hydrographic analyses, etc.), much of which was not included as supporting background data in the proposal? The selection of this solicitation as a research project is justified.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	- As previously mentioned, the proposal is very ambitious, and this is best reflected in the approach. Overall, the approach is well designed but could have been augmented by the addition or consideration of several factors, which are discussed below in more detail. - The approach is feasible. - Results, provided that they are interpretable (particularly the utilization of a composite fingerprinting methodology (isotopes, grain size, elemental composition, mineral magnetics, etc.)) will certainly add to the base of knowledge. The initiation of a pilot study that applies their composite fingerprinting methodology to the source areas and sinks would have served as a validation or proof of concept, lending more support for the funding of a large and holistic proposal such as this one. - Although stated throughout the proposal, there are no novel or "new techniques" here, other than a relatively new model for using 210-Pb to determine sediment chronology. If successful however, this work could assist in or develop independently a progressive way to determine which hydrologic events

may mobilize significant amounts of sediment in this region. - Again, if successful, then yes, this information would be of use to decision makers. -- Other points of interest regarding the stated approach: -- Investigators state that floodplain storage and remobilization of Hg laden sediments represents a much more important reservoir as compared to bed load sediments in these rivers, yet no comparative sampling or analyses of these two compartments is articulated? -- Although the importance of grain size dependence is well known to the investigators based on their text, citations and supporting information (Hg v. % clay for example), it seems that considerable effort is to be spent analyzing bulk sediment? While it is understood that XRF and mineral magnetics are useful essentially only for sands and coarser, why devote so much time, attention and ultimately, resources to a portion of the sediment that will not carry or be associated with Hg? Also along these lines, how applicable will the composite fingerprinting technique they describe be when it is well documented that downstream fining will seperate source sediments based on energy regime (and therefore grain size), a process which in itself should change the composite signature? This complication is not addressed in the proposal but could certainly prove troubling in the interpretation phase. -- Investigators repeatedly mention the importance of Hg methylation, but make no mention of investigating the speciation of Hg? This, as they note, will be very important in the lower, floodplain reaches of the study systems. Total Hg determinations are important, but speciation work is integral in their prospects of successfully addressing their objectives in terms of human risk. Also, with all the sediment work they propose, the quantification of POC (particulate organic carbon) was conspicuously absent. Work in this area (papers by G. Gill and K. Choe, among others) show the importance carbon plays in providing a setting suitable for the methylation of Hg. Documenting total Hg is an important step, but it

	is the methylated form that poses the greatest threat to organisms, including humans.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	- Overall, the work is well documented and the data they intend to provide are feasible given their resources. There are however, several points that could/should be addressed to provide for an improved effort (see approach comments). - The combined experience in this type of work and in particular, this regions hydrologic and geomorphic setting evdienced by the research team is impressive. This suggests a high likelihood of success. However, there are several ways in which the methodology could be improved. The composite fingerprinting in particular, while a powerful tool, is not applicable in all settings. The presentation of some supporting data for this approach would have significantly augmented the proposal's likelihood for success in my opinion. - The scale is large and the problem very complex. I am certain that one large, three year grant will not be sufficient to answer all of the questions this proposal articulates. It will likely provide some very useful information as well as plenty of supporting data to allow for future, targeted funding to address this proposal's deficiences or presently unforeseen complications.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

#0090: Identifying and Tracking Sediment–Adsorbed Mercury Through the Lower S...

Comments	– No. While a great deal of text is spent in spelling out the relationships between mine spoil sediment stratigraphy in floodplains/bypasses and historical hydrologic records, I found no discussion of event based sampling? This monitoring was expected based on the initial project description and I am surprised it was not included with some detail in the proposal. – Regarding treatment-control comparisons, the aforementioned lack of any supporting pilot data regarding the applicability of their composite fingerprinting technique in this system is surprising. This would have served as proof of concept and could also have addressed the problem of downstream fining altering sediment signatures based on how they define them here. – The investigators clearly discuss how they intend to interpret the data and to develop user friendly means to disseminate it to the scientific community, the decision makers and the general public.
Rating	good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	– If successful, the products developed from this work would clearly be of value to scientists, decision makers and the general public. – The investigators outline the design of a spatial database that incorporates all of the data collected as part of this effort, and leaves the door open to continuing to build upon it after the conclusion of the work for this grant. The offering of this database on the world wide web is also noted and encouraged. – Again, proof of concept comes up. Certainly there will be useful data derived from work of this type, but whether it is interpretable given the broad context or not is unknown. If successful, this work would provide a wealth of information on sediment transport in these

	systems, which could then be used as a predictive platform (if storm of x duration and y intensity hits at location z, how much sediment should we expect to be mobilized?). This success would also allow for improved management and understanding of sediment associated contaminants, like Hg.
Rating	good

Additional Comments

Comments	- I believe this work is very important and that the resources of the people should be spent on the best possible experimental design and science. I concur with the reviewers quoted from NSF regarding the similar proposal rejected previously in that if this solicitation is not funded in this program, it should be further refined and re-submitted.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	- The track records of the investigators are very good. The team is heavily biased toward fluvial process and sediment transport expertise. - The team is well qualified, however the inclusion of a more robust biogeochemical component and expertise would have been an asset. - Based on what information is provided in the proposal and available on the internet, describing the capabilities of the investigators, their research facilities and those of their host universities and agencies, they appear to have the necessary infrastructure to successfully accomplish their stated tasks.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	- Overall the budget is reasonable and adequate. - Minor point, if the existing GPR equipment is adequate to the task, as described in the proposal, why is 43k itemized in the budget to purchase a new one?
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	- My summary rating is based on the average score of each rated category presented. Overall, this proposal is very ambitious, attempting to tackle a complex problem with limited presented background data to substantiate the proposed methodology. The researchers have extensive experience in this region, focused on similar problems, their capabilities and those of their respective laboratories and institutions are certainly sufficient to provide the data they propose to collect.
Rating	good

Technical Review #3

proposal title: Identifying and Tracking Sediment–Adsorbed Mercury Through the Lower Sacramento Valley

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals of this project are to evaluate the sedimentological history of hydraulic gold-mining sediment in the Central Valley of CA. This goal is in-line with stated goals of CALFED, particularly as the sediments relate to mercury contamination, and the movement of these contaminants over time. Some of the ideas and goals of the proposed work are timely and important (particularly the movement of mercury in the channel network); some of the goals are not as critical.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The work is justifiable relative to existing knowledge of sediment movement in watersheds, with the exception of the use of numerical models (see approach section below). The conceptual model of the sediment movement in the network is sufficient, and logical. The existing and pilot data point to their research being a logical next step in working on the problem. In particular, Allan James' work is the basis for the

	`bulk of the proposed work.`
Rating	`very good`

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	`I am OK with the bulk of the approach the authors have taken, but I do not think that they have taken a sufficiently predictive approach to the problem. Their proposed methods will draw heavily on mapping existing sediment deposits, coring these deposits, and using hydrologic data to reconstruct when these sediments were deposited. They will use the combination of this data to estimate what magnitude-frequency of floods were responsible for the observed sediment deposits.`
	`I have a few concerns about this approach. First, as the authors acknowledge, there will substantial lag times between when sediment is eroded from the hillslopes and when it is deposited on the floodplain, as the sediment must be routed through the watershed. Thus, a large flood will likely not be responsible for causing sediment deposition if the sediment is not available to be transported (i.e., it is still up in the headwaters). The link between hydrology and sediment deposition is likely to be extremely non-linear, as the source and sink of sediment is completely de-coupled. I highly doubt that the authors will be able to use hydrology coupled with sediment records to determine what floods were important because of the temporal-spatial complications of sediment routing through the system. Perhaps they could use historical records of channel geometry to get at this, but if there is insufficient data, I am worried that the results will be mis-leading.`

	Second, the mercury part of this story almost seems like an afterthought. While sediment routing is indeed important, the movement of mercury is the real story for managers. There is a tremendous amount of focus being placed on sediment movement, but only a limited part of the work will look at how mercury is moving in the system. Is mercury completely immobile apart from sediment movement? In order to characterize mercury hazards, do other forms of movement need to be identified and examined? Third, there isn't a strong predictive modeling component to the proposal, which is something that I think would really strengthen it. The authors mention doing hydraulic modeling, and tying this in to sediment transport modeling, but the specifics are not mentioned. I think that traditional sediment routing modeling would drastically improve the usability and utility of this overall study, as the results could be use to forecast when the sediment currently stored would be moved downstream. At a minimum, I would have hoped that the authors would have provided more detail on the modeling they expect to do.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	I think that the project, as it is currently proposed is very feasible and realistic. The approach and goals that the authors have articulated are appropriate.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control

comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	There will not be active monitoring (a weakness of the proposal I think). There will only be documentation of the historic sediment deposits.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The main product will be a GIS-based map of sediment deposits and mercury concentrations. This will have some utility in the future. I think that a predictive model would strengthen the research value of the proposal, and the predictive model would also be a great product to have at the end of the proposed work. I think, for a project of this type, having an end result be a GIS map and some publications is not reaching out to the potential users enough.
Rating	fair

Additional Comments

Comments	The authors have not demonstrated a link to management agencies or industry that would use the developed information. The end result of the proposed work, a series of papers and a map, may not be highly usable if the authors are not working along managers during the project. I would hope that they could pull into their research team someone or a group that has a vested interested in the project results, and have that person or group work alongside the authors (i.e., attend planning meetings) so that they can maximize the utility of the final project.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	`I am not overly excited about the research team, primarily because it is heavy on the geomorph and light on the mercury and contaminant transport. Four of the five PIs are geomorphologists, with primary experience in sediment movement. James' experience at the site is particularly valuable and key to the success. This is a substantial proposed project, with many components, a large budget, multiple PIs, etc. While I don't think the PI should be penalized for being right out of his PhD, I think that a 1 million dollar project is a big project to cut your teeth on in terms of management. I appreciate that Singer has several years of experience, but not at this project level. Dunne is there to help, but he hasn't been allotted a large portion of the project (actual no budget going to Dunne). I am not sure why Dunne is actually on the proposal, as he doesn't seem to be given any role in it. I would also prefer to see a larger role given to someone with experience in contaminant transport rather than just sediment transport (in addition to Slotton). Basically, the budget invests heavily in sediment, and partially in mercury transport, when mercury transport is the governing factor for the utility of the project. Some addition of someone with expertise in contaminant routing, rather than just sediment routing, would strengthen this team.`
Rating	`fair`

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Aside from the concerns raised above in the capabilities section, the budget appears reasonable.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	This is a strong and interesting proposal. I would prefer to see a more predictive and modeling based approach so that the results of the project can be used for prediction rather than just description. I would prefer a stronger jointing with an outside group that has a vested interest in the work, and that this outside group be an active member of the project so that the results are highly usable at the end.
Rating	good